

UNITED STATES OF AMERICA



FOUNDED 1836

WASHINGTON, D.C.

EXPERIMENTS AND OBSERVATIONS

ON THE

ABSORPTION OF ACTIVE MEDICINES

INTO THE

CIRCULATION;

SUBMITTED,

AS AN INAUGURAL THESIS,

TO THE EXAMINATION OF THE

REVEREND JOHN EWING, S. T. P. PROVOST;

THE TRUSTEES

AND MEDICAL FACULTY OF THE UNIVERSITY OF PENNSYLVANIA,

ON THE EIGHTH DAY OF JUNE, 1801,

FOR THE DEGREE OF DOCTOR OF MEDICINE.

BY BENJAMIN G. HODGE,

OF THE WEST-INDIES, HONORARY MEMBER OF THE
PHILADELPHIA MEDICAL AND CHEMICAL SOCIETIES.

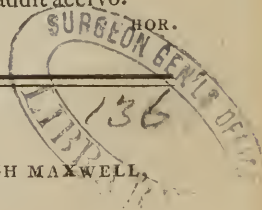
Ore trahit quodcunque potest atque addit acervo.

PHILADELPHIA:

PRINTED FOR THE AUTHOR, BY HUGH MAXWELL,
COLUMBIA-HOUSE.

.....

1801.



TO CASPAR WISTAR, M. D.
ADJUNCT PROFESSOR OF ANATOMY, SURGERY, &c.
IN THE
UNIVERSITY OF PENNSYLVANIA,
THIS ESSAY
IS DEDICATED
BY
HIS VERY OBLIGED FRIEND,
AND VERY GRATEFUL PUPIL,
BENJAMIN G. HODGE.

Coxe, with the Compliments
of the

Author



TO BENJAMIN SMITH BARTON, M. D.

PROFESSOR OF

MATERIA MEDICA, NATURAL HISTORY, AND BOTANY,

IN THE

UNIVERSITY OF PENNSYLVANIA, &c.

WHOSE FRIENDSHIP

I AM PROUD TO POSSESS, AND ANXIOUS TO CHERISH;

AND WHOSE APPROBATION,

OF

THIS ESSAY,

HAS BEEN,

TO ME, THE MOST FLATTERING CONSIDERATION.

INTRODUCTION.

WHEN I first conceived the idea of writing on the absorption of medicines, I determined to take up the subject purely in an experimental point of view, without any reference or regard to the arguments that have been adduced, to prove either this or that thing. I observed the difficulty of drawing a conclusion upon many points involved in the subject, from such facts as are in our possession: so much are these facts opposed to one another, and so great the plausibility attached to each. This being the case, I was anxious to make some attempts of my own, to arrive at truth; and I was certain, it was only (if at all) to be found, by making a direct and impartial appeal to the only true authority, to Nature herself. It soon appeared, however, that the subject I had chosen, was much more than I could possibly do justice to; was more copious than I at first imagined, and would lead

to a much more numerous train of experiments, than the term allotted would permit me to make.

The difficulty, likewise, of procuring subjects for experiments, was a matter of no inconsiderable obstruction to our progress. The reader, therefore, will perceive, that we have advanced but a little way, and that too, in a field which has already been trod by others. The ground they have passed over, however, well deserves a more accurate examination. We can flatter ourselves only with having attempted this, by repeating a few of their experiments, and by shewing the proper degree of credit that ought, in our opinion, to be attached to others; and if, after all, the reader should be dissatisfied, let the trouble and inconvenience of experimenting, answer for our deficiencies. As it will be impossible, and even unnecessary, in the following dissertation, to notice every thing that has been said and done upon the subject, we shall relate only those particulars that appear to be the most important; and in the course of such relation, we shall introduce the experiments we ourselves have made, together with such observations as may occur to us upon the subject.

The man, who would attempt to teach, or write, on subjects connected with physiology, without

the aid of facts and experiments, had better relinquish the matter altogether. Here it is, by these guides, and not conjecture, that we must be conducted to truth. “Of all the discoveries that have been “made concerning the inward structure of the human body,” says an elegant writer, “never one was made by conjecture. Accurate observations of anatomists have brought to light innumerable artifices in the contrivance of this wonderful machine, which we cannot but admire, as excellently well adapted to their several purposes. But the most sagacious physiologist never dreamed of them till they were discovered.” It is to these guides, therefore, that I mean to commit myself, in the following inquiry. But, alas! what dependance can be placed upon the greater number of facts that are to be found in the records of medicine? Who can draw the difficult line, that separates truth from error, in most of those particulars, that have been dignified with the name of facts? We shall, perhaps, in the following dissertation, have occasion to observe, that the term has been much abused, and that it has but too often been employed to designate the dreams and errors of the imagination.

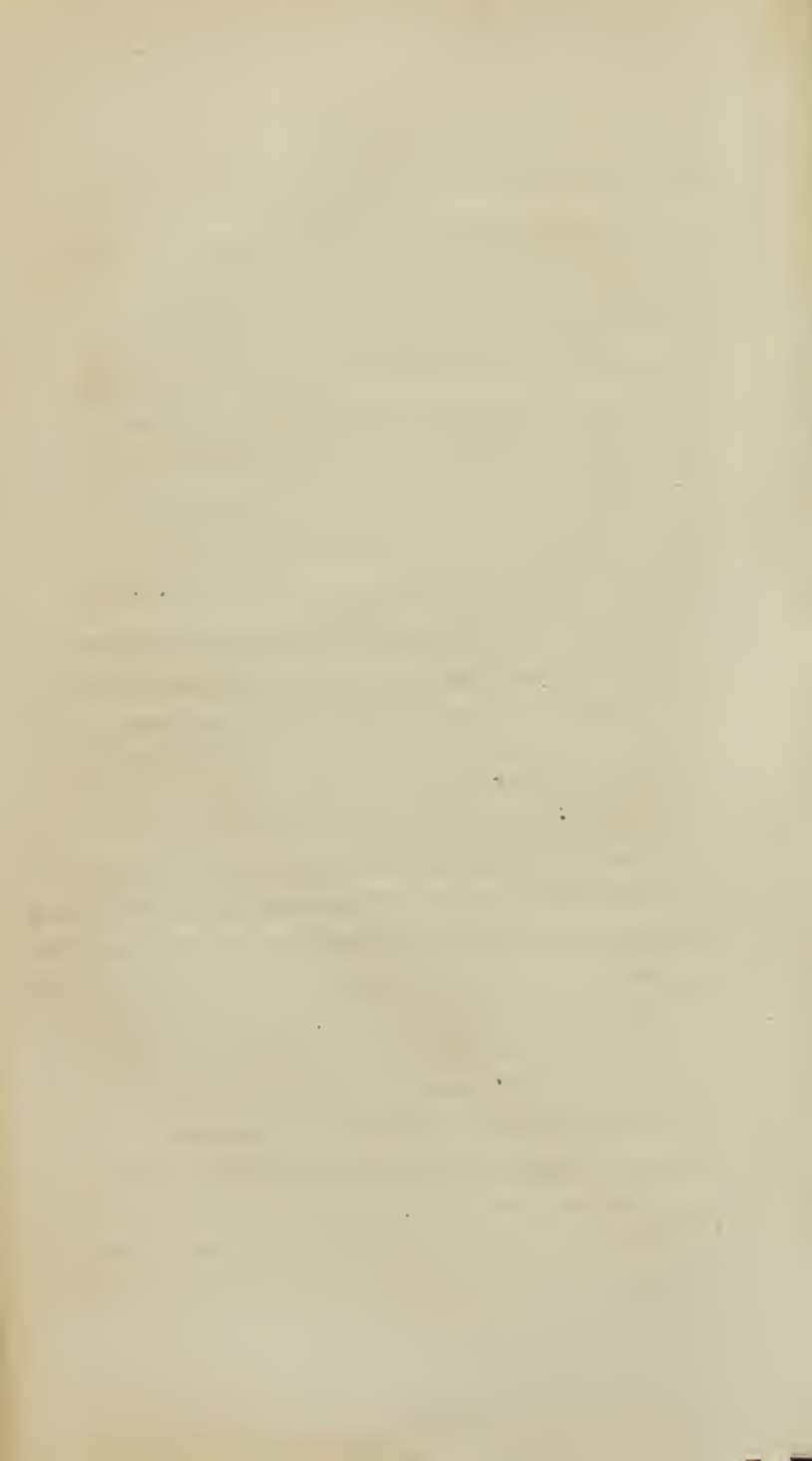
It would be unnecessary to offer any preliminary observations on the nature of the question,

that is to be agitated. The importance of the subject, in a practical, and its beauty in a physiological point of view, will be too obvious to the physician and philosopher, to need the aid of eulogy to recommend it. Its utility will at once be made apparent, by referring the attention of the reader to the treatment of a common intermittent fever. In this disease, for example, if it could be correctly ascertained, whether the Peruvian bark exerts all its influence in the alimentary canal without going any farther; or, whether it is absorbed into the circulation, and is there enabled to produce in part its salutary effects, the practice, resulting from this knowledge, would be materially different. In the former case, the practitioner would throw in the medicine, only on the approach of a paroxysm; but, if the latter position were true, the patient would be obliged to take the medicine with no, or but little variation in quantity, during the whole of the intermission. A similar remark would apply to many other cases; and it is certainly owing much more to an ignorance in the *where*, than it is in the *how* medicines operate, that so much difference has prevailed concerning the time and mode of exhibiting them.

One thing I must request the reader to observe, which is, that the following experiments were not

made, as is sometimes the case, to bend to the theory of the experimenter. On the contrary, when we entered upon the investigation of the subject, and had nearly advanced to the conclusion of it, we still believed in the absorption of active medicines: but, from the results of our experiments, and for other reasons which shall be given in the sequel, we have been necessitated to relinquish our former opinion. Whether the reasons assigned for so doing are sufficient, will be determined with more propriety by the reader.

We are conscious to what danger we expose ourselves, in attempting to oppose a doctrine that is become venerable by time, by talents, and by industry. But, from you, illustrious professors! (to whom I now address myself) from you, we have little to apprehend; for, from a long acquaintance with you, both in a public and private way, we have much reason to believe, that prejudice forms no part of your characters; and that you are always willing to listen, with candour, and indulgence, to any attempt, the ultimate object of which is truth, that “jewel, which all good and wise men,” together with yourselves, “are in pursuit of.”



EXPERIMENTS AND OBSERVATIONS

ON THE

ABSORPTION OF MEDICINES, &c.

THE notion, that medicines, in order to be effectual, must be absorbed into the circulation, in their active states, took its rise at an early period. It was the very soul of the humoral pathology. As this doctrine referred all the phenomena of disease, to particular dispositions of the fluids, so it was imagined, that medicines could do no essential service, unless they were applied to the seat of the disease. These theories have been, in a great measure, corrected: the former, by a more improved pathology; the latter, by a nicer attention to the effects of medicines on the body. In consequence of this accurate attention, physicians soon discovered the insufficiency of the above explanation; and found themselves obliged to seek for some mode of accounting for several particulars respecting the operation of medicines, more rational and satisfactory, than the one by absorp-

tion. That exquisite, though mysterious law of the animal œconomy, by which one organ is rendered sensibly alive to the feelings of another; by which impressions on one part, are immediately propagated to others, the most distant in the body, presented itself to their consideration; and this principle, under the various names of *vis medicatrix*, consent, sympathy, &c. has long been considered as a chief agent of the business in question.

But, although the influence of this sympathetic connection has been acknowledged, physicians have never departed entirely from the old doctrine. They have always continued to believe, that medicines, in general, are absorbed into the circulation; and that this, in many cases, is even necessary to their proper action. Thus Dr. Cullen, who was a great advocate for the notion of sympathy, nevertheless observes, “In many cases of increased evacuations, it is indeed pretty evident, that the medicines, exciting the evacuations, are actually conveyed and applied to the secretories or excretories of the parts concerned:” And, the absorption of mercury into the constitution is, with many, deemed absolutely necessary in the cure of *lues venerea*; not to mention many other instances.

This opinion, however, is at present totally denied by some physicians, who attribute the effects of all medicines to sympathy alone.

The supporters of this heresy, among whom I may include myself, declare, that no active medicine is ever taken up into the circulation.

That opium, camphire, &c. cannot, nor have they ever been proven, to circulate, as camphire and opium, in the blood vessels of a living animal. They think it would be incompatible with the life of the animal, that such active substances should be absorbed into the circulation; since milk, and other bland fluids, are known, when injected into the vessels, to occasion immediate death. They argue further, that, as the powers of sympathy are acknowledged, it would be useless and unphilosophical to admit of more causes than are sufficient to explain the phenomena; and that it would, moreover, be contrary to the general simplicity of nature, who never employs two instruments to accomplish a single purpose. This new theory, therefore, resolves itself into the following form, and supposes, that every medicine, "When received into the stomach, after the first impression on the very sensible coats of that organ, the nature of it is gradually changed by the solvent powers of the gastric juices; or, if incapable of being digested into a mild and nutritious chyle, it is carried through the intestinal canal, and ejected as useless and noxious to the body."* X

These objections are founded, not on hypothesis, but in matter of fact; and if the inutility of a thing, be a proof of its falsity, we should have little trouble in establishing our point, by shewing from many circumstances, that absorption is not only

* Percival, Operation of Medicines.

X How then does Opium injected into the rectum, produce its effects on the

insufficient, but that it is perfectly unnecessary, in explaining the action of medicines. The following facts would suffice for this purpose:

When the body is in a state of langour and debility, one glass of wine, will immediately restore to the muscles their accustomed tone; will revive the drooping spirits, and will add new force and vigour to the whole arterial system. A tremor of the hands in drunkards is often lessened, or removed, by a dram, or some strong wine; and that, in so short a time, as entirely to preclude the possibility of absorption. Some medicines, as camphire and opium, have been found in the stomach, without any diminution in quantity, long after they have produced their peculiar effects. “Frogs,” says Girtanner, “which live a long time after the heart is cut out, and which are consequently deprived entirely of blood, are killed as quickly by the poison of the viper, as if their blood had not been let out.” And the celebrated professor Whytt has ascertained, by experiment, that if the heart of a frog be taken out, and a solution of opium injected into the abdomen, the animal will be convulsed in a very short time. Dr. Barton, in his valuable lectures on the *materia medica*, says, he once stopped an hemorrhage in a distant part by the exhibition of only two grains of *sac. saturni*. Medicines not only can be shewn to produce their effects on distant parts of the body, by a mere impulse upon the part to which they may be applied, but there are many, whose ope-

rations are alone explicable upon this principle; many that can act in no other way; a striking instance of which is to be found in the cure of buboes, hernia humoralis, &c. by emetics. We might thus go on to multiply proofs of the same nature, and to shew, that the effects of every class of medicines, as sialagogues, diuretics, diaphoretics, &c. are all referable to a sympathy between the different parts of the system. But, perhaps, we have already insisted too much upon the establishment of a principle, which few people, in the present improved state of medicine, will be hardy enough to deny.

Some gentlemen, more partial to their own theory than to the facts of others, have endeavoured to raise objections to the experiments, concerning the injection of milk and other matters into the circulation, and to destroy the true deductions that ought to be formed from them. This they do, by observing, “that what passes along the lymphatics or lacteals, is carried into the thoracic duct, and there mixed with a large portion of chyle or lymph, by which its acrimony is sheathed and diluted, or its chemical properties changed, before it enters the blood.”* Such objections, however, do not, by any means, appear to be sufficiently founded. For, in the first place, simple dilution does not alter the nature or properties of any substance whatever. A given quantity of wine will

* Percival.

produce similar effects, whether it be taken into the stomach in a diluted, or undiluted state. And, in the second place, as it is well known, that the chyle mixes with, and becomes blood itself, very shortly after it is poured into the blood vessels, the milk, and other foreign matters united with it, would then be deserted, and would be left in that same undiluted state, in which they have been injected into the vessels, and in which they kill. As to the chemical change, which these foreign matters are supposed to undergo, in the lacteal and lymphatic vessels, that will be readily admitted. It is just one of the propositions we wish to maintain. We wish to establish, that all matters, capable of forming chyle, must be reduced to that state previous to their entrance into the circulation; and if they will only grant this, they shall have our free consent to call the change, a chemical one.

If, therefore, any substance causes death, &c. when injected immediately into the blood vessels, it follows, of necessity, that that substance, provided it be in the same form, will always produce the same effect, let the mode of introducing it into the circulation be ever so much varied. We will avail ourselves of this reasoning on a subsequent occasion, to extricate us from a difficulty. We do not mean, however, to insinuate from this, that some other substances may not be thrown into the blood-vessels, and may not exist there without much danger. This inference would be opposed by facts, and one of a very remarkable

*a Glass of Brandy taken into the Stomach
 & produce Intoxication; although the same
 when diluted with water, will not produce*

nature is related by the celebrated Fontana*; but such experiments only prove the possibility of the absorption of those substances, and with possibilities we have nothing to do.

The supposition, that active medicines are taken into the circulation, appears to owe much of the credit it possesses, to the very mechanical manner in which the lymphatics are supposed to absorb and transmit different articles into the blood.

Not being acquainted with any other modes of explaining the rise of fluids in tubes, but by capillary attraction, and the formation of a vacuum, philosophers availed themselves of these principles, to account for the same phenomenon in the absorbents. The explanation was a very natural one. Unfortunately, however, they forgot, in this, as in many other cases, that they were reasoning, not on dead, but living matter; and that analogies, drawn from the one, would not always be applicable to the other. Thus it is not only difficult to imagine how the soft, delicate, and yielding extremities of these vessels can form themselves into capillaries, &c. but, from many circumstances, it is rendered pretty clear, that they evince something like design in their operations. They shew a greater aptitude to receive some substances, than others, that are reduced to the same form. They absorb the nutritious, in preference to the other contents of the alimentary canal, which would not

* Fontana on Poisons.

be the case, if they acted purely on those mechanical principles. Neither will these principles explain the manner in which they carry away the hardest parts of the body, such as bone, cartilage, muscle, &c. unless, indeed, we were to assume what is not proved, viz. that these parts undergo a solution previous to their absorption. But the amazing power of the chylo-poietic viscera, among which the lymphatic system must be included, appears to be the most material objection to the absorption of medicines, in their active forms. From that almost nameless variety of substances, which the caprice or necessity of man has induced him to take as aliment, only one combination results, viz. chyle. The absorbents are capable of selecting, amidst so great a variety, such principles alone, as they can afterwards convert into chyle. The chyle of different species of animals, differs in no respects, although their aliment is, apparently, the most opposite in its nature. The absorbents appear, further, to be the most pre-eminent of the chyloform organs, for they not only produce chyle, from such matters as have suffered the action of the stomach, (for chyle is not formed in the stomach or intestines) but they possess the power of absorbing, and operating the same change upon milk and other nutritious articles, that may be injected into the different cavities of the body, as the rectum, the abdomen, &c. In these cases, the absorbents appear to have the whole business of digestion in their own hands. This chyloform power in the

lymphatics, may be shewn, by an analogy derived from the animal and vegetable kingdoms. The whole tribe of vegetables are intirely destitute of a stomach. Some animals, of an inferior order, are likewise formed without this organ. Now, in both these living productions, there can be nothing to digest their food but the absorbents; and yet, the alimentary matters, they take in, are most certainly digested.

The only proofs, then, upon which the absorption of medicines rests, are the appearances of those substances, in the secretions, and in different parts of the body, after they have been taken into the stomach, &c. Facts of this nature are plentifully scattered over every work that relates to medicine. Indeed, I recollect but very few substances, not only in the *materia medica*, but even in Nature herself, that have not, at one time or other, been discovered to pass through the circulation. But it must not be supposed, that whenever an author relates facts of this kind, that he always does it upon the evidence of his own senses. As the absorption of medicines was universally believed, and looked upon as a medical axiom till lately, physicians copied from one another with as much confidence, as if the circumstances they related were actually before their eyes. Thus, although few men in modern times, have had the happiness of witnessing, as Etmuller did, the presence of “ simple water wine, and wine with sugar, and emulsions,” in the identical unchanged state, in the

urine, in which they were taken by the patient; yet, we find his facts frequently mentioned, and that too, by some of the most respectable modern physicians. As some of these facts, however, are handed down under the protection of names, that will always command our respect and veneration, we must now proceed to consider them.

There was no subject wherein a greater unanimity prevailed among physicians, than that iron and its various preparations, were absorbed into the circulation. The opinion was grounded upon many plausible circumstances. Iron, it was well known, existed as a component part of the blood, and was supposed, likewise, to be the cause of its red colour. It was, and, I believe, continues to be, a general observation among practitioners, that this colour is increased by the use of martial medicines. Mr. Menghini even found, by experiment, that the blood of persons, who take martial preparations, contains more iron than it does in an ordinary state; and Mr. Lorry observed, that the urine of a sick person, to whom he administered iron in a state of extreme division, was coloured with the nut-gall.* Many modern physicians have altered their creed upon this point, chiefly in consequence of an experiment made by the ingenious Dr. Wright, wherein this gentleman was unable to detect any iron in the thoracic duct of a dog, to whom he had given an ounce and an

* Chaptal.

half of sal martis.* Such contradictory experiments produced much doubt in my mind, especially as I was induced to believe, that, if any foreign matter could gain admittance into the blood, except in the form of chyle, it would certainly be iron. In order, therefore, to make up an opinion on the subject, I had recourse to the following

EXPERIMENTS.

One drachm of green vitriol, finely powdered, was wrapt up in some meat and given to a dog. In about an hour and a half after this, he vomited a little; but whether he threw up the whole quantity, I cannot decide. I suspect he did not, since so considerable a time elapsed, before the vomiting commenced. However, as soon as I perceived this, I dissolved four drachms of the same medicine in a quantity of milk, and injected it into his rectum. In the course of half an hour afterwards, I had the misfortune of seeing this voided likewise. I then dissolved near an ounce more of the medicine, in some more milk, which I again injected into the rectum, taking, at the same time, effectual precautions of preventing a return of it. In an hour and an half after this, the dog was killed, and several drachms of chyle were obtained from the thoracic duct. To one portion of this was added some of the prussiate of potash; to another, the

alcohol of galls. No change of colour, however, ensued in either case. But when I dissolved the sixth of a grain of the vitriol, in a portion of the same chyle, my tests discovered the presence of it immediately. Quantities of the serum and urine were tried in the same manner, but without discovering the presence of iron. Mr. Jacobs assisted at this experiment, who agreed to every thing I have related.

EXPERIMENT.

Having procured a dog, and kept him starving for several days, I poured down his throat two scruples of a solution of sal-martis; at the same time, half an ounce of the same medicine, dissolved in a sufficient quantity of milk, was injected into his rectum. In the space of fifteen minutes, the dog vomited a little, and was affected with a violent tenesmus, without being able, however, to discharge the contents of the rectum. In about an hour and a half the dog was killed, and the thoracic duct secured by a ligature. After the duct became distended, I punctured it, and collected a quantity of chyle sufficient for the purposes of experiment. This being divided into separate portions, suffered no change of colour, upon the application of the different tests formerly mentioned. A white coagulum always took place in the chyle when tested by the alcohol of galls. After the experiment was finished, and we were about

to leave the room, the propriety of examining the mesenteric glands, was suggested by an ingenious friend. For this purpose, I detached a number of these glands, which being cut through, in order to expose their internal surface, were immersed, in separate portions, in clean water. To these glands, thus circumstanced, were added sufficient quantities of the alcohol of galls, and the prussiate of potash. Soon after, a black colour in the one, and a blue in the other, took place, evincing thereby, the presence of iron. At first, I supposed this circumstance might have been owing, either to the knife made use of in dividing the substance of the gland, or to some particles of green vitriol accidentally insinuating themselves into the water. That it did not depend, however, upon this latter cause, must appear evident from this, that the colours produced, were confined wholly to the substance and interior structure of the glands.

But to be thoroughly satisfied upon the subject, I repeated the experiment above half a dozen times, taking care to avoid the knife, and every circumstance that could influence, or could be adduced as an objection to the experiment. But the results were the same in every instance. Did I perform the experiment too soon for the iron, thus absorbed, to pass through the gland, and to arrive at the thoracic duct? Would it not have discovered itself in the duct, if sufficient time had been allowed? This looked extremely probable, and a wish to see how far it was true, brought about the following experiment.

EXPERIMENT.

Having kept a dog starving two days, I offered him half a drachm of green vitriol, dissolved in a portion of milk, a part of which only he took: half an ounce of the same medicine, dissolved in another portion of milk, was injected into his rectum. After remaining three hours and a half, he was killed, and a quantity of chyle collected from the ductus thoracicus, which did not produce by the addition of the usual tests, any colour, that was in the least indicative of the presence of iron. Our next object, was to examine the mesenteric glands: and here the same phenomenon occurred, as in the last experiment, only the colours were more intense in the interior structure of that part of the mesentery which intervenes between the glands and intestines. The liquor of the thoracic duct exhibited a very different appearance in this, from what it did in the preceding experiments; instead of a white milky colour, it resembled serum that is slightly tinged with the red part of the blood. To ascertain whether this iron in the glands was really owing to absorption, or whether it was the mere effect of transudation taking place after death, I had recourse to another experiment.

EXPERIMENT.

Half an ounce of green vitriol was injected into the rectum of a dog. Two hours afterwards he

was killed, and immediately, before transudation could take place, several of the mesenteric glands were taken out. Separate portions of these were tried by the different tests, while another portion was put into simple water to serve as a comparison. A black colour was produced in that gland to which the alcohol of galls was applied. It was not quite so evident as in the former experiments. No change of colour took place in that portion tested by the prussiate, except in a small part of the mesentery, which adhered to it. That portion of gland placed in water suffered no change. Some chyle was taken from the thoracic duct, but it was not found to contain any iron. I then wished to see if the black colour produced in this experiment, were the effect of the alcohol of galls, independent of any iron.

EXPERIMENT.

Some glands were taken from the mesentery of a dog, which had served for an experiment of a different kind, but which had taken no iron. These, however, suffered no change of colour, by the application of the alcohol of galls to them.

With regard to the absorption of the iron, as far as the glands, it does not appear that any weight can be attached to the circumstance. For it must be recollected, that in every instance an immense derangement was brought on the intestines; the colon and rectum always looked as if they were fast

tending to gangrene, and in some cases they were actually in that state, being as black as my hat.

These experiments, then, not only prove the absence of iron in the chyle, but likewise, that of the acid with which it was combined; for, if the sulphuric acid had been present, it would have formed a blue colour with the prussiated alkali. But it may be objected, that they are inconclusive, inasmuch as they “only evince, that iron did not subsist in the chyle as a vitriol, qualified to strike a black colour with galls,” &c. This objection may be an ingenious one, but it certainly is not founded in a knowledge of chemistry. For, contrary to the assertion of Dr. Percival, if iron had existed there, either as iron, or as an oxyde, or in any other form, it would certainly have been discovered by the above means.

But how are we to reconcile the experiments of Menghini and Lorry, formerly mentioned, and likewise, the more intense colour of the blood, observable in patients under the use of iron? In answer to this, I must take leave to observe, that these facts, even when admitted in the fullest extent, will not, by any means, establish that opinion, which, on superficial examination, they are so eminently calculated to do. The subject, respecting the existence of iron in the blood, is well known to be involved in much obscurity. How its presence there, is to be explained; what purpose it answers; how far it is subject to variation, in point of quantity; and, if subject to variation, with what par-

particular states of the system, an increase or diminution, in its quantity, may be connected? are queries, which cannot be answered in our present imperfect knowledge of the business; and until they are, these experiments will be of little value. It is not our business to account for the presence of iron in the blood, but it is most probable, that it is a compound substance, formed by a peculiar process of organization, and is intirely independent of absorption. In this way, it appears to be formed in vegetation; for experiments shew, that it exists in vegetables, that have fed entirely on air or distilled water*. It is pretty evident, however, that this iron may exist in very different quantities at different times. Indeed, why cannot this take place with the iron, as it does with regard to the other component parts of the blood? The relative proportions of these are well known to be constantly changing. The serum is at one time abundant; at another, it exists in small quantity. The red globules are now more, now less; and the same thing is true of the coagulating lymph. Iron may, therefore, be found in the blood in larger proportions at particular times, and that too, when the patient is under the use of iron: yet it does not follow, that this increase in its quantity is necessarily owing to the absorption of the medicine. The use of martial medicines may only be the indirect cause, by producing that state of the system on which an

* Chapthal, Abernethy,

increase of iron in the blood depends. As to the colour of the blood becoming redder by the use of iron; I think this is more liable to objection than the question we have just been considering. If the colour of the blood really depended on iron, the circumstance, I acknowledge, would go a great length to prove the absorption of this metal. But sufficient evidence is still wanting to shew the truth of the position. The greatest physiologists, 'tis true, have agreed in attributing the redness of the blood to the iron it contains; "but when we reflect how many various colours iron gives in its various states; when we reflect, that the unknown cause, which gives colour to the iron, may give colour to the blood; when we reflect, that of this crocus of iron, we can hardly procure one poor grain from four hundred grains of these red particles of the blood; we cannot but be conscious, that this peculiarity is not yet explained*." Besides these objections, it may be presumed, that the improvement, both of the appetite and of digestion, and a more vigorous circulation, all which result from the exhibition of iron, are circumstances as satisfactory in explaining the phenomenon, as any unknown cause that could be alleged.

It is unfortunate for medicine, that theory is so often mistaken for truth, instead of being considered as the mere creature of uncertainty.

* John Eel.

Despairing of the possibility of ever introducing iron into the circulation, I determined to make the attempt with some other substances.

The power of stopping hemorrhages in distant parts of the body, and some other particulars, respecting the operation of astringents, have entitled this class of medicines, in the estimation of many, to the privilege of being absorbed into the circulation. To arrive at some degree of certainty on the subject, the following experiment was instituted.

EXPERIMENT.

Half a drachm of the powdered nut-gall, concealed in a quantity of meat, and three ounces and a half of a saturated aqueous solution of the same medicine, mixed with some milk, were taken by a dog, that had been starved for several days. It may be necessary to observe, that the dog did not vomit, nor did he appear to be affected in any respect whatever. About two hours afterwards he was killed, and a large portion of chyle collected from the thoracic duct. But this did not exhibit the least appearance of astringency, by adding to it a solution of the sulphate of iron. The chyle, in this instance, could have been formed only from the aliment which I had just given the dog, and with which I had combined the galls. Notwithstanding this, however, it did not contain a particle of the medicine

Among the many substances, concerning the absorption of which, physicians have been generally agreed, lead must be mentioned. The insidious manner in which this metal undermines the constitution, favours much of absorption. The change of colour which it causes in the muscles of paralytic limbs, has likewise been supposed to depend upon the same principle. And professor Thunburg, who was salivated by accidentally taking a large quantity of ceruse, mentions, that the lead was perceptible in his saliva.* It were to be wished that the professor had been a little more particular, and had ascertained the presence of the lead by some better criterion than that of taste. But although these circumstances are not at all conclusive, yet they were sufficient to create a strong suspicion in my mind that I should find them realized by experiment.

EXPERIMENT.

I kept a dog confined, and whenever I fed him, I mixed a portion (generally about 15 grains) of *sac. saturni* with his food. The dog bore this regimen very well at first; but after some time he grew sick of the medicine, and commonly threw it up, so that I was obliged to desist. By this practice, I forced him to take, altogether, not less than sixty grains, exclusive of that which he vo-

* Voyage to the Cape of Good Hope.

mitted up. Half an ounce of the same medicine was immediately injected into his rectum, and proper precautions taken to prevent its expulsion. After two hours had elapsed he was killed, and a quantity of the liquor of the thoracic duct collected, which did not shew the presence of lead, when tested by the phosphoric acid, and the sulphurated hydrogen gas. Neither did the serum of the blood evince the existence of lead in it by the same tests. Here lead did not exist in the chyle or blood, either as sugar of lead, or as an oxyde, for if it had been present in those states, it would have been rendered sensible. The glands of the mesentery did not contain any of the medicine.

My next object was, to ascertain whether mercury could be found to enter the circulation, since it is upon this subject particularly, that the doctrine of absorption has rested its greatest support.

The authorities in favour of the absorption of this metal, are numerous and nearly as respectable. Our credulity, indeed, is often startled, especially when we are informed, that upon opening a vein some drachms of it have flowed out with the blood,* &c. But, after all, it must not be denied, that cases of a similar nature have been handed down to us by men of great eminence in the science. Bœrhaave, Haller, Mead, Brödbelt, and a long catalogue of others, have *seen* globules of mercury in the bones. Added to this, is a case, related

* Mead's Works.

by Dr. Cooper, of a woman who was salivated, producing the same affection in her sucking child. This last fact, however, deserves very little consideration; for, if mercury was used externally in any form, that circumstance alone would be sufficient to explain the mystery. But in opposition to the above facts, we have the experiments of Dr. Slare, which were made on the saliva of a lady in a state of salivation without discovering the smallest quantity of mercury.† Dr. Saunders, “by various and accurate tests, could not discover, in the secretions, any mercury in persons under a salivation, either from the internal or external use of it.”‡ Some experiments of a similar kind, which I have made, were marked with similar results; but as they may answer some useful purposes, and may probably tend to guard others from a deception which I was near falling into myself, I shall relate them.

EXPERIMENT.

Several ounces of saliva were procured from a patient in a high state of salivation. In this were immersed several pieces of gold, silver and copper, which were allowed to remain two days. At the end of that time, no amalgam was formed on them. Supposing that if there were any mercury, it would more readily unite with other metals, when it was in a state of vapour,

† History of the Royal Society. ‡ Saunders on the Liver.

(EXPERIMENT.)

I took the same saliva, and after suspending a bit of gold just above the surface of it, it was subjected to heat and entirely evaporated; but no amalgam appeared on the gold. I was apprehensive that these experiments were made on too small a scale. To obviate this objection:

(EXPERIMENT.)

A quart of the same saliva, being slowly evaporated to a thick consistence, produced no change of colour in a bit of gold that was immersed in it a whole night.

EXPERIMENT.

By the favour of I obtained several ounces of blood from a patient, in a high state of salivation. After it had separated into serum and crassamentum, I placed in the bottom of the bowl containing it, a piece of the purest gold, and allowed it to remain there a whole night. But no amalgam was formed on it.

EXPERIMENT.

A part of this blood, being placed in an oil flask, with pieces of gold and copper suspended on the surface of it, was exposed to heat. The gold

did not suffer any change,¹ but in a short time, the copper became as white as silver, and some persons, very capable of judging, declared it to be the effect of amalgamation. As the gold, however, was not affected, some doubts still remained, which, I thought, could only be cleared up by a comparative experiment.

EXPERIMENT.

I, therefore, obtained some blood from a person, who was not under the use of mercury, and by treating it as in the last experiment, the same appearance precisely took place. The copper was whitened. Had not faith in the experiments of others, and some degree of scepticism, induced me to make this trial upon the blood of a healthy person, I should have been most miserably deceived.

The deduction, then, that must be formed from these and other experiments, is, that mercury does not exist in the circulation or secretions in a pure metallic state. But they go no further; for, if mercury had been present, either as an oxyde, or in combination with an acid, it is evident, it would not have been detected by the means made use of; and proper experiments, with a view to find it in those states, have not, as far as I know, been made.

But do not the depositions of fluid mercury (supposing for a moment that such was ever the

case) in the bones, &c. shew that it must have existed in that form in the circulation? I answer, no. For, that it does not, is evinced by what has been said; and that it cannot is proven by a fact, related by Saunders, who injected two drams of mercury into a vein; and thereby killed the dog, that was the subject of the experiment.* If such depositions can be accounted for at all, it must be by supposing, either that the mercury is reduced from its combinations in passing through the extremities of the arteries, or, that this takes place after these combinations have been deposited. The only thing that remained, was to ascertain, whether mercury existed in the states just mentioned; for which purpose the following experiment was made.

EXPERIMENT.

I procured a glass tube, hermetically sealed at one end and open at the other. Into this was introduced several drams of the dried blood and saliva of the same patient. A bit of gold was placed over a small hole, left in the open end of the tube, to prevent any mercury from passing off undetected. The tube was then exposed to a red heat, and after remaining there a sufficient time, we could not discover that any mercury had either passed off in vapour or was present in the tube.

* Saunders on the Liver.

The contents of the tube exhibited much the appearance of globules of mercury. This appearance occurred likewise in a former experiment on the blood, and, in both instances, was near leading me into an error; but when attentively examined, they were found to be globules of air instead of mercury. So great was the resemblance, that these globules were, at first sight, considered by many, even the professor of chemistry himself, as mercury. Will not this serve to explain those appearances, which, on dissection, have been attributed to mercury? "Heaven knows how seldom things are what they seem!" One thing I am certain of, that if any person, prepossessed with the belief that mercury is absorbed, had seen these experiments, he would, without doubt, have pronounced them to be globules of mercury. I shall conclude this subject with a quotation from Mr. Hunter. "It may be supposed unnecessary to mention, in the present state of our knowledge," says this great man, "that mercury never gets into the bones in the form of a metal, although this has been asserted by men of eminence and authority in the profession; and even the dissections of dead bodies have been brought in proof of it; but my experience in anatomy has convinced me that such appearances never occur."*

* Hunter on the Venereal.

Another circumstance, that has been urged by the supporters of absorption, is that animals, which are at one time innocent and wholesome, will become poisonous, in consequence of eating certain poisonous matters. The fact, I believe, is strictly true. We ourselves have witnessed it in the fish called the sprat, in the West-Indies, and in confirmation of the same thing, may be found, in the memoirs of the London Medical Society, an excellent paper on the subject, by Dr. Thomas. This author, in speaking of the poisonous quality of the sprat, and some other fish in the West-Indies, observes, “that it arises, however, from their food is strongly corroborated by several circumstances; but what that food is, remains yet to be discovered. It is a well known fact, that the land-crab (*cancer-terrestris*), when taken near manchineel trees, is found, particularly in dry seasons, at one time safe, at another, poisonous, from feeding on the bark or leaves of that tree in lieu of other nourishment.” Pheasants are said to acquire a poisonous quality, by eating, from necessity, the fruit of a certain plant. A fact, of the same kind, is related in Foster’s Observations, during a Voyage round the World; where part of the crew, and some domestic animals, were poisoned by a particular species of fish. “Sometime afterwards,” says the writer, “I was told, that a fish of the same species was caught at Tanna by some of the sailors, who salted it and eat it, without any ill effects; whence it is to be supposed, that this

species is not poisonous in itself, but only from the food, which it accidentally meets with, &c.”

These authorities, without mentioning others, are sufficient to establish the truth of the matter. Taking the fact, therefore, for granted, let us see what can be made of it. At first, we were inclined to look upon this as one of the strongest proofs that could be brought forward in support of the absorption of medicines; but, upon reflection, it appears to us, that it ought to be taken with considerable limitation, and that for the following reasons:

When fluids, or other matters in a state of solution, are enclosed in different cavities, they cannot, in a living state, be removed from those cavities, but by natural or artificial passages, or by absorption. The gall-bladder may be distended to a great size; the urinary bladder may be enlarged to an almost incredible extent, so as to be mistaken for dropsy; the liver may be filled with pus; yet, they never suffer any part of their contents to pass through into the abdomen. Indeed, if it were otherwise, it would not only endanger the life of the animal, but would altogether supercede the necessity of having reservoirs in the body for particular fluids. As soon however as death takes place, the case is altered. These reservoirs immediately loose the power of retaining their contents, and transudation is the consequence. This fact is very familiar to anatomists, who find the bile, the urine, &c. transuded,

in almost all their dissections. Agreeable to this, it is easy to conceive, that poisonous matters, existing in the alimentary canal, may, after death, transude through and affect the muscles and other parts with their deleterious qualities; and might thus impose upon us the belief that they were absorbed during life. This is not speculation; but is rendered more than probable by the following words of Dr. Thomas, the same sensible writer, whom we have quoted above. Speaking of the poisonous fish in the West-Indies, he remarks, “ When taken off the hook, if the precaution is used to gut and salt them *immediately*, they seldom or never create any disorder. The following facts evidently prove this. Mr. Henry Buckley, treasurer of the island of St. Kitts, is extremely partial to the barracuta (*perca major* of Brown), and never refuses to purchase them from fishermen whom he knows, provided they gut and salt them as soon as they are taken out of the water. He has never met with an accident since he adopted this practice, which is now upwards of thirty years.” Again: “ A fisherman caught some yellow-bill sprats in Halfmoon-bay, and, after using the above precautions of gutting, &c. threw the entrails into the sea, for fear of poisoning his favourite dog, which accompanied him in all his excursions: this happened in the morning. He carried the sprats home, and, together with his family, dined on them; in the afternoon he returned to his usual occupation of catching sprats, and observing

his dog busily eating something, which it had picked up in the surf, he had the curiosity to examine what it was, and found it to be the guts of the fish thrown ashore by the waves. He immediately afterwards observed his dog in great agonies, and soon after he carried him home he died." The above is related from unquestionable authority, and can be confirmed by the testimony of several of the most respectable inhabitants of St. Kitts. Another fact, equally important, happened to a Mr. Duport at Palmetto-point. This man has been a fisherman more than forty years, and employs a number of negroes in drawing seines in different parts of the island. They one day caught a great quantity of yellow-bill sprats, which, as usual, he forbade his negroes to make use of, to avoid accidents; but, contrary to his orders, they gutted a number of them, and threw the guts on a dung-heap in an enclosed yard, where he kept his poultry. To his astonishment next morning he found a great number of them dead, but, on inquiry, none of his people were affected*." Other instances of the same nature are related, which I omit.

These facts are very important, and very much to our purpose. They prove, that the poisonous property of these fish is owing, as has been supposed, to their food; likewise, that they can be rendered innocent by taking out the stomach and

* Memoirs of the Medical Society of London.

intestines, *as soon as they are caught*. This circumstance, I apprehend, is only to be explained upon the principle of transudation; for, if these poisonous matters, found in the stomach, &c. could be absorbed into the circulation, and afterwards diffused through every part of the body, it would be impossible, that the above, and improbable, that any other, precautions whatever, would be sufficient preventives against the effects of the poison. Whenever, therefore, any animal, otherwise wholesome, is found to poison, in consequence of a particular kind of food, I would attribute it to the cause already assigned; or else to carelessness or mismanagement on the part of the cook. This coincides with the intention of Nature, who ordains, that the aliment of every animal should be decomposed; should be formed into chyle; in short, should become a part of the animal itself.

Does sulphur ever enter into the circulation? This has been answered in the affirmative by some, and their assertion is grounded on the following reasons. They say, that as “sulphur, whether externally or internally used, produces a cure in the itch;” so, in either way, they presume, its operation to be similar*. In other words; when sulphur is taken internally, they suppose, it is absorbed, and, by means of the circulation, is applied, in its active state, to the seat of the disease on the surface of the body. Secondly, they assert,

* Percival, Operation of Medicines.

that “ sulphur tinges the silver, that may be worn by the patient of a black colour ; and that it communicates its odour to the perspiration.”

These circumstances, if true, would, no doubt, carry much weight ; but, at present, we suspect that they carry more inaccuracy and deception with them than any thing else. For, that the internal use of sulphur will produce a cure in the itch, is a position not altogether so completely established as these gentlemen would make us believe. Many physicians deny it, and in particular we have the authority of Dr. Bonomo, who has paid much attention to the disease, and has thrown more light on the nature of it, than any other person. His words are, “ Neither do inward medicines perform any real service in this case, it being always necessary, after a tedious use of these, to have recourse to other external ones, already mentioned*.” But granting, what is not probable, that sulphur, taken internally, is a cure for the complaint in question ; even then the fact would not be a sufficient proof of its absorption. For sympathy, which is so active an agent in all the concerns of health and disease, would deservedly claim a large part of the credit. “ In cutaneous diseases we should remember, that the stomach may only be sympathetically affected ; and that such disorders may be cured by the operation of medicines on the stomach†.” That sulphur will tinge the silver, which may be about

* Philosophical Transactions Abridged, vol. 5.

† Jackson's Medical Sympathy.

the patient, will not be denied ; but the fact will be of no use to us in the present controversy, unless we could be certain (which is not the case) that the patients, in all such instances, were strictly confined to the internal use of the medicine. It is well known, that sulphur, in a state of vapour, is one of the most penetrating substances ; so much so, that a small quantity, rubbed on any part of the body, will, by the application of heat, traverse every other. A person, merely by handling some of the sulphur-ointment, and afterwards going near a fire, had his sleeve-buttons rendered completely black. But independent of the foregoing objections, the absorption of sulphur is rendered very improbable by experiment. A gentleman in this city, whose talent for chemical research is very great, could not discover any sulphur in the blood of a patient whose system, to use a common phrase, was completely saturated with the medicine. I gave a man, a large quantity of the hepatic water, which, like other mineral waters, is said to pass off, by the urinary passages ; but was not able, by means of the acetite of lead, to detect the presence of any sulphur in the urine.

Among others, the presence of carbonic acid in the urine has been urged as a further proof of the absorption of medicines, and when this fact comes to us from so great an authority as Dr. Priestley, it is no wonder that so much stress has been laid upon it. It is, however, by far the weakest argument, that could be selected. When

carbonic acid is united with other substances, it must exist either in a state of chemical combination, or in that of simple mixture. In the former case, the attraction is stronger, but in the latter, the acid is retained by a very slight force, and can be driven off by a low degree of heat; much lower, indeed, than the natural temperature of the human body. It is owing to this circumstance, that, when porter, cyder, &c. is taken into the stomach, the natural heat of the part produces a disengagement of the carbonic acid, and this is either belched up immediately, or else, is carried forward, and discharged per anum. This consideration renders it impossible that carbonic acid should ever exist in the blood, in a state of mixture; for, not only the heat, but the very agitation of that fluid, would be sufficient to expel it, and occasion death. Whenever fixed air exists in the urine, the same consideration makes it necessary, likewise, that it should be in a state of chemical combination with the earthy and alkaline bases, present in that excretion, otherwise, it would produce a flatulency, and consequently, a disease of the bladder. If the reader will only look at Dr. Priestley's experiment, he will find that this is perfectly correct; and that what the doctor took for carbonic acid was actually in a state of combination; for, "it must be observed," says he, "that it required several hours to expel this air by heat; and after the process, there was a considerable sediment at the bottom of the vessel. This

was, probably, some calcareous matter, with which the fixed air was united*,” &c. The urine of a person, who was constantly kept on the use of two or three bottles of porter a day, did not afford me any carbonic acid, when exposed to a heat sufficient to expel it, if it had existed there in the state of simple mixture. Let us then, for a moment, suppose, that Dr. Priestley, to use his own words, has, “ more than once expelled, from a quantity of fresh made urine, by means of heat, about one fifth of its bulk of pure fixed air, as appeared, by its precipitating lime in lime-water, and being almost wholly absorbed by water†.” I ask, if this can be a sufficient proof of the matter in dispute, when we know, that an alkaline carbonate exists, naturally, in all urine? Carbonic acid is secreted from almost every part of the body, and its presence in the urine is no more a proof of its absorption than the presence of the lythic, the phosphoric acids and ammoniac, &c. is an evidence, that these matters were absorbed.

The following experiment proves, that all urine will exhibit the same phenomena that occurred in the above mentioned experiment of Dr. Priestley.

EXPERIMENT.

A quantity of common urine was placed in an oil-flask and exposed to a boiling heat. The air

* Priestley on Air. † Ibid.

that escaped was made to pass, by means of a syphon, through lime-water. The first air that came over was nothing but the common atmospheric air of the vessel; but the next portion produced a large precipitation in the lime-water, and was greedily absorbed by water. Upon pushing the experiment a little further, the precipitate appeared to us to be in part redissolved, and there was, likewise, a very abundant white sediment, formed in the flask. At first, I had not the smallest doubt that it was the carbonic acid, which was given over, both in this, and in Dr. Priestley's experiment; but it struck me, that if it had been this acid, it never could have been driven from its combinations with lime and the fixed alkalines, by the boiling heat; nor did it appear to be united with ammoniac. I, therefore, began to suspect that it was not the carbonic acid; and I found my suspicion most handsomely confirmed by the next experiment.

EXPERIMENT.

A fresh quantity of the same urine with that used in the above experiment, was treated exactly as in the former case; but as soon as I perceived, by the precipitation in the lime-water, that the proper air was coming over; I removed the lime-water, and placed, in its stead, a clear solution of the acetite of lead. Here a copious white precipitate, immediately took place. This convinced

me, that it was not carbonic acid; for, if it had been such, no precipitation or change would have been made in the lead-water. As the muriatic, the phosphoric, and the lythic, acids are known to exist in urine, it must have been one of those salts.

We learn, then, that Dr. Priestley's experiment is correct, but that his deduction from it is wrong. The air, which came over in his experiment, certainly resembled fixed air, in precipitating lime-water, and in being absorbed by water; and no person, without further experiments, could have possibly avoided the error into which this illustrious character has fallen. These experiments, though few, will, I flatter myself, be satisfactory to every person, and will be sufficient to set aside every thing that has been said concerning the absorption of carbonic acid. Lastly, it may be useful to mention, that the precipitate, produced in urine, by lime-water, is not, as has been advanced, any proof of the absorption of carbonic acid; for all urine, that I have examined, will exhibit the same phenomenon.

We are told, by Mr. C. Darwin, that he discovered the presence of nitre in the urine of a friend, who had taken about two drachms of that salt*. As this experiment has excited a good deal of curiosity, and has contributed to confirm the belief of many in the absorption of active substances, to pass it by unnoticed, would, perhaps, be a mark of neglect; I, therefore, determined to repeat the ex-

* Zoonomia.

periment. But, before doing this, I found what I thought a satisfactory refutation of his pretended fact, in the following

EXPERIMENT.

To a pint of urine was added a drachm of nitre, and after it had completely dissolved, I soaked a bit of paper in the solution, and afterwards allowed it to dry. Upon placing it in the flame of a candle, it did not shew the least appearance of nitre. I then mixed two drachms of nitre with the same quantity of urine, but was unable to discover this by similar means. I gradually increased the quantity of nitre, until I carried it as far as three drachms and a half to the pint of urine, and with this quantity I was but just able to detect it; perhaps, I should not have done it, even then, had I not been aware of its presence. The truth is, that Mr. Darwin's experiment was as hypothetical as the theory it was intended to support, and he deserves as little credit for the one as he does for the other.

“ Fingere qui non visa potest ;

hunc tu,” medice, “ caveto.”

So far, then, as our experiments go, we have seen no reason for admitting the absorption of medicines. We proceed, in the last place, to answer, in a general way, some facts, which have been deemed very important, and no less decisive, against our opinion. One would be almost

tempted, indeed, to believe in the absorption of active medicines, not only from a great body of plausible facts on the subject, but from the rage, that has always existed among physicians, for finding out another and a shorter passage to the bladder, than that of the general circulation. Their object was, to explain, by such a passage, the sudden appearance of the properties of those matters in the urine, that had been taken into the stomach; and it is, at least, reasonable to suppose, that these men were, in some measure, convinced of the reality of the fact, before they would set themselves to theorize about the probable cause of it. This circumstance, I may observe, would seem to be nearly as much in favour of absorption, as it is opposed to the notion of a new passage; for it is certain, on the one hand, that the properties of many substances do occasionally evince themselves in the urine; but although this new passage has existed so long in the imaginations of physicians, yet it has, very unfortunately, never been found to exist in the body. Without wishing to avail ourselves of the advantage of this imaginary passage, I have mentioned it, merely to shew, that the qualities of bodies do exhibit themselves in the urine, and other secretions of the body; and as the fact is established by so much testimony, it must be admitted. Neither am I going to assert, that this fact has no weight at all. On the contrary, our senses, the only instruments of knowledge, are extremely limited in their operations,

and, although it is a reflection humbling to our pride, it is nevertheless true, that they convey into the mind, not the essence, but only the qualities of matter. The qualities of substances are the only criteria by which we can judge of the presence of those substances. Indeed, it is perfectly impossible to disconnect the ideas of substance and quality, or to imagine that they can exist, the one, without the other. Some of the properties of medicines, as smell, colour, &c. cannot, therefore, be supposed to exist in the secretions, without some material agent to produce them; and so far, the case is pretty clear. But, while we maintain this kind of language, we must not imagine, that the presence of one or two qualities, peculiar to any medicine, is an evidence of the presence of that medicine in its intire form. By a natural, though unfortunate, association of ideas, however, the matter has been otherwise considered. The whole has been judged of, from a part only; and opium, rhubarb, &c. are said to enter the circulation, because the odour of the one, and the colour of the other, are found in the secretions. Thus, while I am looking at a picture before me, containing the head of a great and good man, all his virtuous qualities are seen on, and are inseparable from, the picture; and I almost fancy myself in company with the great original. But it is well known, that all medicines, with which we are acquainted, are compound substances, possessing various sensible qualities, some of which reside in the one, some in the other, of their component

parts. It is equally well known, that many, and, for aught we know, the greatest number of these articles, are fitted to administer nourishment to the body. Thus opium contains a gum that is nourishing; stramonium is eaten by the goats and other domestic animals; and the willow forms the bread of mankind, in some parts of the world; together with many other instances that might be mentioned. Nature knows no characteristic distinction between medicinal and esculent vegetables. They all appear to be composed of the same elementary principles, and the whole difference lies in the name. Medicines, therefore, like other aliment, when taken into the body, according to the power of the digestive organs, are decomposed. Some parts of them will be rejected as useless, others will go to the formation of chyle. These nutritive parts, it is presumed, will, even in the form of chyle, retain some of the sensible qualities which they possessed in the state of combination, and will thus be carried into the circulation. So that, whenever the qualities of medicines appear in the secretions, the only thing they prove is, that the nutritious parts of those medicines were absorbed. This may be illustrated by many facts and examples.

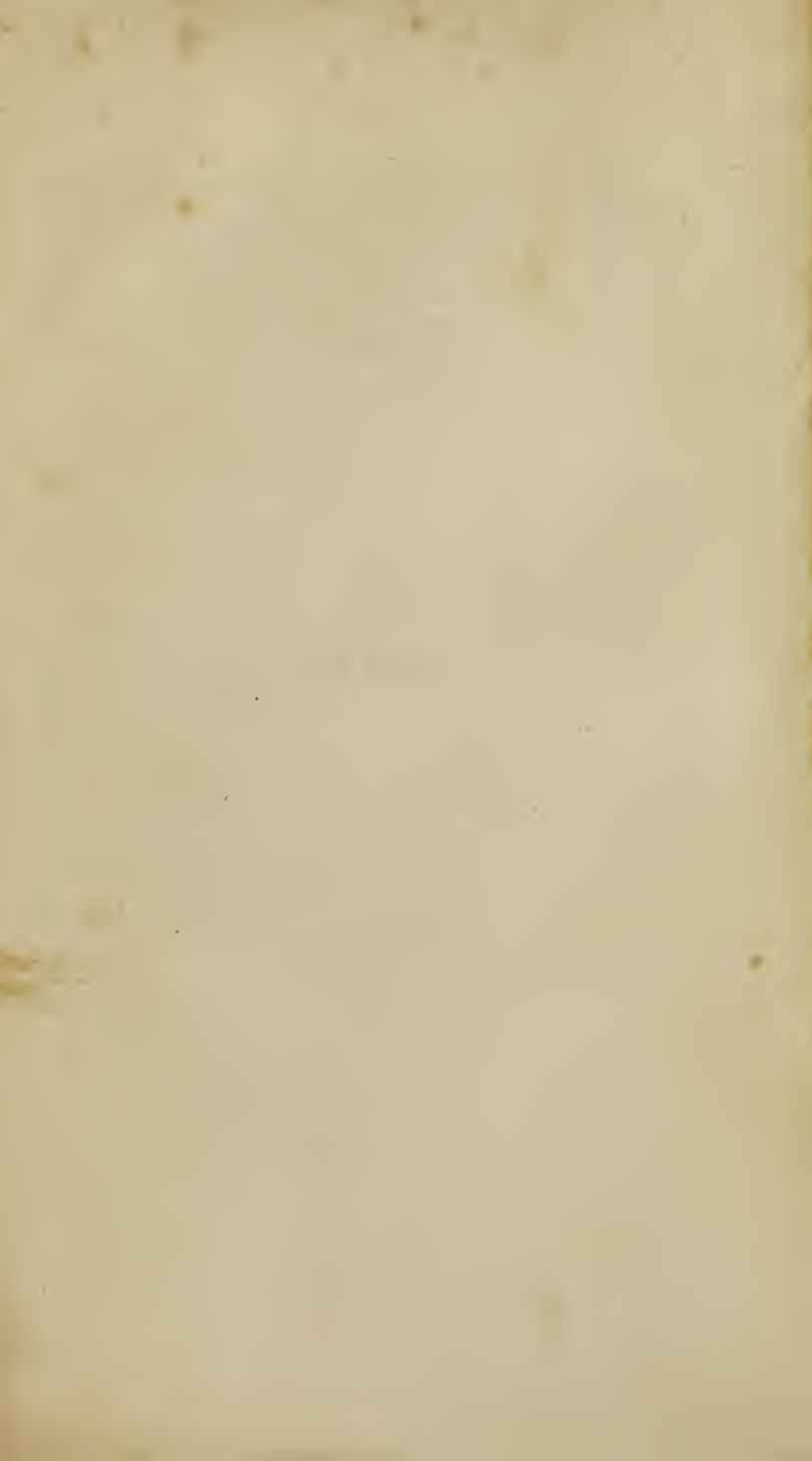
Thus sea-birds, have their flesh sedgy, from living intirely on fish; yet, no one will believe, that the fish is taken up and swims, as fish, in the blood-vessels. A circumstance, much in point, is related by Dr. Percival. “ Extract of logwood, taken

internally," says this ingenious physician, " sometimes gives a bloody hue to the urine. But the astringency of it does not, according to my trials, accompany its colouring matter*." In like manner, certain articles of food, as the different species of the siliquosa, the asparagus, the garlic, the indian-fig, &c. all communicate their smell or colour to the urine; yet, do we not know that these vegetables nourish the body, and, to answer that purpose, do we not know that they must be formed into chyle? Madder communicates its colour to the bones, &c; yet, do we not know, that madder contains principles that are nutritious? The turkey-buzzard feeds on putrid animal matter, and it is said, that this is absorbed as putrid matter, because, forsooth, the feathers of these birds have a stinking odour! Now, when we reflect, that these birds are always up to their eyes in putrid matter, and always surrounded by a putrid atmosphere; when we reflect, that putrid matter, according to Spallanzani's experiments, will be rendered sweet by the action of the gastric juice upon it, and when we reflect, that this putrid flesh is not only nourishing, but that an animal will be killed, if only one drachm of putrid serum be injected into its vessels, we cannot but be surprised, that a fact of this nature should be looked upon as a proof of absorption. Let the reader extend this principle, and he will be able to explain many other facts.

* Manchester Memoirs.

We have thus considered the most interesting particulars in favour of the absorption of medicines, and shall conclude, with this observation to the reader : *si quid novisti rectius istis ;* which I do not doubt, *candidus imperti : si non, his utere mecum.*

THE END.



Med. Hist.

WZ

270

H688e

1801

C.1

